plane. The hyper-elliptic integrals of this more general problem are interpreted in a similar way to the elliptic integrals of the previous discussion. From the nature of the case, in these lectures, an outline sketch of a large subject is all that can be given, but the lines are traced by the hand of a master; and for filling in the details we must look to the author's treatise, "Ueber die Theorie des Kreisels," which is now in course of publication by Teubner.

William Stokes, his Life and Work (1804-1878). By Sir William Stokes. *Masters of Medicine.* Pp. 256; plate i. (London: T. Fisher Unwin, 1898.)

THE memoir before us is an interestingly written account of a man whom all physicians respect. Stokes was a master of medicine, and the inclusion of his biography in this series shows the wisdom of the editor. The name and work of Stokes are perhaps not as well-known to the modern student of medicine as they ought to be; this is probably due to the fact that not sufficient time has passed for us to appreciate his work, or rather for us to estimate its great value. He worked and taught at the time when exact methods of physical diagnosis were beginning to be applied by the clinician. Pathological chemistry and bacteriology were practically non-existent, and clinical thermometry was in its infancy. The work of Laennec on the stethoscope had attracted the attention of medical Europe, and opened up the enormous field of the correlation between physical signs and symptoms. It is in this particular field that the work of Stokes was done, and his treatise on the diagnosis and treatment of diseases of the chest still remains a classic. With the exception of Laennec's work, which it considerably amplified, this book must be regarded as one of the most noteworthy upon this subject which had until then been written.

To turn from his professional to his private life, the letters which are given us in this biography show us Stokes as a cultured Irish gentleman, forming the centre of a wide circle of friends. The biography is carefully written, and will appeal to all those who are interested in that epoch of the history of medicine to which its subject belongs.

F. W. T.

Practical Organic Chemistry. By George George, F.C.S. Pp. 94. (London: W. B. Clive.)

THERE is no date on the title-page of this book, but the preface bears the date May 1898. No scientific book should, however, be published without the year of publication being printed upon the title-page.

The book is intended "for the elementary and advanced examinations of the Science and Art Department." It contains a few experiments on the detection of common elements in organic compounds, on melting and boiling points, organic acids, alcohols, sugars, &c., notes on the methods of examination of mixtures containing organic compounds, and on the preparation of some reagents used in organic analysis. The volume will thus make the student acquainted with the reactions of, and the tests for, common organic bodies.

Food Supply: a Practical Handbook for the use of Colonists and all intending to become Farmers Abroad or at Home. By Robert Bruce. With an Appendix on Preserved and Concentrated Foods, by C. Ainsworth Mitchell, B.A. Pp. xvi + 159. (London: Charles Griffin and Co., Ltd., 1898.)

THIS is the second volume of the "New-Land" Series, edited by Prof. G. A. J. Cole. It is a concise and soundly practical manual of farming in which the fundamental principles of successful agriculture, and of the selection and management of live-stock, are described. It is only paying a compliment to the author to state that the book contains the kind of information published by the Department of Agriculture of the United States, and in such official publications as the *Agricultural Gazette* of New South Wales and the *Agricultural Journal* of the Cape. As we are at present without a central office for supplying information to British farmers, it is the more necessary that the means of education in the science and practice of agriculture afforded by such books as the one under notice, should be widely known. The volume deals with the fundamental principles of most branches of farming, and will prove of service in any part of the world. The forty-nine half-tone reproductions of photographs of representative animals, illustrating the chief breeds of live-stock, will be of particular interest to farmers.

Royal Gardens, Kew. Bulletin of Miscellaneous Information, 1897. Pp. 437 + 68. (London: H.M. Stationery Office, 1897.)

THE well-known *Kew Bulletins* afford the best of evidence of the valuable work done at the Royal Gardens in advising upon possible developments of the natural resources of our Colonies and dependencies. Each *Bulletin* contains a number of plain statements of attempts made to introduce new and commercially profitable plants in suitable districts, of improved methods of cultivation, and of work that men trained at Kew are doing in the various parts of the world to which they have gone from the Royal Gardens. The *Bulletins* issued in 1897 are collected in the present volume, and together they make a worthy contribution to economic botany. Among the contents is a long list of publications issued from Kew during the years 1841-95. This record of accomplished work is an eloquent testimony of the important part which the Gardens take in botanical research, and in developing the resources of the Empire. Several papers on botanical exploration and enterprise are included, and sixty-three pages are devoted to the report of the Royal Commission appointed to inquire into the condition and prospects of the West India Colonies.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Spectrum of Metargon.

WE have delayed in replying to Prof. Schuster's letter in your issue of June 30 in order that we might make further experiments on the subject. We have had the kind assistance of Prof. Schuster, who demonstrated to us the close similarity between the group of green lines in the metargon spectrum and the spectrum of the blowpipe flame. We subsequently satisfied ourselves regarding the similarity of the metargon spectrum and the "Swan" spectrum, shown by carbon monoxide in a vacuum tube. At first sight, Prof. Schuster seems justified in attributing that spectrum to the presence of carbon or of one of its compounds. Yet we think that careful consideration of the following facts will necessitate a suspension of judgment:—

(1) The sample of metargon was mixed with twice its volume of oxygen, and sparked for two hours in presence of caustic soda. This sample, introduced into a vacuum tube after removal of oxygen, still showed the same spectrum.

(2) A little oxygen was introduced into the gas, and the mixture was then admitted to a vacuum tube. Oxygen lines became visible, but no bands of the so-called "carbonic oxide" spectrum. On removing the oxygen by means of phosphorus, the original spectrum appeared with its customary brilliancy.

Thinking it possible that the ordinary spark may not have had a sufficiently high temperature to decompose an imaginary stable carbon compound, a jar and spark-gap were introduced, and sparks passed through a mixture of metargon with twice its volume of oxygen, standing over caustic soda, for six hours. No

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contraction occurred, and the spectrum of the gas was unaltered, after removing oxygen.

(3) An artificially made mixture of carbon monoxide and argon—about equal volumes of each—was mixed with oxygen. It was sparked and exploded. It was then further sparked over soda for a quarter of an hour. On introducing the gas into a vacuum tube, after removal of oxygen, no carbon lines or bands were seen, but only the spectrum of pure argon.

The bands in the green of metargon are exceedingly brilliant, and the spectrum is by no means of the character of a subsidiary one. It does not appear to be possible to enfeeble them relatively to the rest of the spectrum.

We have found it possible, in hundreds of cases where it was necessary, to remove traces of carbon compounds from gases evolved in heating minerals—chiefly helium—to remove the carbon bands by "running" the tube, *i.e.* by increasing the intensity of the current until the aluminium pole melted. The green and red bands, under these circumstances, slowly disappear, and the spectrum of helium or of argon, as the case may be, shines out "clean-cut," and shows as bright lines on a black background. This process is impossible with metargon; no change is produced even after long "running."

We must again call attention to the facts that this gas shows the ratio of specific heats 1·66; that it possesses sensibly the same density as argon; and that it is a solid at the temperature of liquid air, boiling under atmospheric pressure.

Although, therefore, we are the first to admit that the spectrum of this gas requires further investigation, yet, from what we have observed, we provisionally adhere to our original view that it possesses the characteristics of a definite chemical individual.

We would take this opportunity of correcting a misprint in the *Comptes rendus*, cxxvi. p. 1762, where the wave-length 5849·6 is attributed to metargon, instead of to neon.

W. RAMSAY.
M. W. TRAVERS.
EDWARD C. CYRIL BALY.

University College, London, Gower Street, W.C.

Liquid Hydrogen.

PROF. DEWAR'S letter in your last issue is such a pronounced personal attack on me, that I feel I ought to deal with the remarks to my prejudice which it contains, though I will try to avoid imitating its tone.

(1) He refers to the statements on which I base my claim to the invention of the self-intensive method as matter which "has already been refuted." I should be glad to know when and by whom. They are clearly numbered 1, 2, 3, 4, in my last letter, and form the substance of my first. At the Society of Chemical Industry Mr. Lennox, though he was present and heard the statements repeated, with every opportunity of contradicting them, did not do so. Prof. Dewar, far from refuting statements 1 and 3, did not even deny them; and his attack on the second (respecting the novelty of the invention) resulted in strengthening it, since it showed that he was reduced to building up an anticipation by taking material from several different sources, having been unable to find any account of the combination before my proposal in November 1894. The fourth statement had not then been made, as hydrogen had not been liquefied. Where then has the refutation taken place? In both his letters to you Prof. Dewar keeps all four statements at a very respectful distance.

(2) Prof. Dewar uses the words "accusations which he was compelled to withdraw when he met me face to face," and "when brought to book at the Society of Chemical Industry." It is quite untrue that I withdrew anything at all. On the contrary, I said that "I had nothing to withdraw," and that my assertions were "a simple and direct statement of historical facts," repeating more frequently than is shown in the printed report that the facts were exactly as I had stated them. As to what took place between Prof. Dewar and his assistant it is obvious that, not having been present, I could have no knowledge; and I can only publish what I know of my own knowledge, or can prove by conclusive evidence. Deductions from the facts must be made by every one for himself, and I reminded Prof. Dewar that as I had published no such deductions I could not withdraw them.

(3) I was not, at the time of my communications to Mr. Lennox, "convinced of the general dishonesty of Royal Institution methods," as Prof. Dewar suggests. I regarded the Royal Institution as one of the temples of science, and Mr. Lennox as its chief acolyte, who might, perhaps, when my offering had been examined and found worthy of acceptance, introduce me to the favourable notice of higher authorities.

(4) What I am "to be understood as saying in the letters you have published," is so clearly set forth in my four numbered statements in your issue of June 23, that Prof. Dewar's doubts on the point cannot be so puzzling as his question implies.

(5) Prof. Dewar's acquaintance with patent-law cases involving a host of partisan expert witnesses and costly counsel is too extensive and familiar to leave him in any doubt as to the reason why a man without means does not begin a prosecution for infringement. I could, however, warn the infringers; and this I did. The protest having been made, I am still free to prosecute when circumstances render it possible and advisable to do so. and the present prospects of low-temperature work make it by no means unlikely that action may yet be taken.

(6) Prof. Dewar's admission, referring to Dr. Linde's method, which he had just heard described, "that the practicability of such a mode of working had never struck him," was made in the opening sentences of his remarks, without any limiting qualifications, but with express inclusion of both "the mechanical ingenuity and knowledge of thermodynamics" involved; so that its only fair interpretation is with reference to the description that had just been given of Dr. Linde's combination, which is, except in details, the same as mine. The force of the admission is not lessened by quoting a subsequent passage which refers to one part of the combination. Dr. Linde and I had invented a combination which made it possible to liquefy air without using any other refrigerant than water. Prof. Dewar admitted that he had never thought out the whole combination. Whatever therefore he and others had done with some parts of it, when the combination came out he ought to have recognised its novelty, instead of endeavouring to piece it together out of old patents and experiments.

(7) Neither M. Solvay nor Prof. Onnes claims to have invented a combination by which continuous free expansion from a nozzle is able, without using other refrigerants, to liquefy air: so that Prof. Dewar misleads his less instructed readers by putting those gentlemen forward as my rivals on the ground that they claim to have used parts of the combination.

My communications to Prof. Dewar's assistant were, however, of earlier date than any publication of Dr. Linde's process. This is the fact of which, with its corollaries, I had hoped to obtain a frank admission from Prof. Dewar, and I would have much preferred that the discussion in your columns had been confined to the points raised in my first letter. Prof. Dewar, however, instead of frankly admitting my claims, as other prominent scientific men have done, or discussing the statements on which they are based, has seen fit to give his attention almost entirely to the more personal elements in the controversy. In two letters he has called my action "dubious" and "not straightforward," and has said that either I am "a singularly dull person" or am consciously imposing "upon the credulity of the world," that I contradicted myself "when brought to book," and that I "was compelled to withdraw accusations" which in fact I explained that I had never made, while refusing to withdraw anything at all. Under these circumstances I think that few of your readers will blame me for asserting the justice of my claims, though I regret that so much of your valuable space should have been occupied by matters of this nature.

W. HAMPSON.

July 1.

The Distribution of Prepotency.

NO numerical estimate appears to have been made of the frequency with which different grades of prepotency are distributed. Breeders are familiar with the fact that certain animals are peculiarly apt to impress their personal characters upon offspring, but how frequently and to what extent this tendency occurs has never, I believe, been investigated. The following attempt is therefore of interest, though not free from objection in minor details. In *Wallace's Year Books* of the American Trotting Horses, lists are given (1) of the sires of offspring, any one of which has succeeded in trotting one mile in 2 minutes and 30 seconds or less, or who has "paced" (= ambled) the same distance in 2 minutes and 25 seconds or less; (2) of the dams of at least two such offspring, or else of one such offspring and one such grandchild. A selection was made from lists (1) and (2) of sires and dams who were them-

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